Reply to “Comment on ‘Inference with minimal Gibbs free energy in information field theory’ ” by Iatsenko, Stefanovska and McClintock

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We endorse the comment on our recent paper [Enßlin and Weig, Phys. Rev. E 82, 051112 (2010)] by Iatsenko, Stefanovska and McClintock [Phys. Rev. E 85 033101 (2012)] and we try to clarify the origin of the apparent controversy on two issues. The aim of the minimal Gibbs free energy approach to provide a signal estimate is not affected by their Comment. However, if one wants to extend the method to also infer the a posteriori signal uncertainty any tempering of the posterior has to be undone at the end of the calculations, as they correctly point out. Furthermore, a distinction is made here between *maximum entropy*, the *maximum entropy principle*, and the so-called *maximum entropy method* in imaging, hopefully clarifying further the second issue of their Comment paper.

Iatsenko, Stefanovska and McClintock [1] point out two possible sources of confusion about how the minimal Gibbs free energy method which was introduced in our paper [2], has to be used.

The first point of confusion concerns the usage of temperature in the Gaussian approximation of a high dimensional probability density function. This was introduced to enable us to emphasize regions near (temperature $T < 1$) or more distant from ($T > 1$) the maximum by giving them a larger weight in the approximation. Our original claim is still correct in that the signal mean can be read off approximately from the mean of this tempered Gaussian. If the uncertainty information is also extracted from the tempered Gaussian, the tempering has to be undone at the end of the calculation, as was pointed out correctly in [1]. This was actually implicitly done in our paper in Eq. (44) and very indirectly indicated in the sentence enclosing this equation. However, we fully agree with [1] that this important fact should have been spelled out more prominently.

The second point is the slightly deceptive usage of the term *maximum entropy* (ME) in our paper, which is defined there as the unconstrained maximum of the Boltzmann/Shannon entropy. This term should not be confused with the *maximum entropy principle* (MEP) [3], which is the constrained maximization of such an entropy, a distinction correctly made by [1]. Our aim in using the terminology of ME was to highlight the importance of incorporating phase-space volume factors correctly into inference schemes, which counteracts the attraction of the posterior maximum and thereby impedes overfitting.

We might have given the impression that we were arguing against the usefulness of the MEP by discussing critically, but only briefly, the often used *maximum entropy method* (MEM) in imaging. In hindsight, it might have been wiser to put this discussion into a separate publication, so as not to risk further confusion. In any case, we want to explain the difference here.

The MEM imaging can be derived from the MEP in the specific case in which the image is composed of a nearly infinite number of uncorrelated and in principle distinguishable flux elements of an infinitesimally small strength each [4]. In this case, each possible image can be attributed to an internal image entropy, due to the number of ways the flux elements can be redistributed among pixels without changing the image. MEM can be shown to be a maximum a posteriori method, with the internal entropy prior suppressing large flux values stronger than exponentially [2]. Although the conditions under which MEM is optimal are rather artificial and rarely met for the problem at hand, its application is widespread. However, ME and the MEP are more general, in that they not only deal with the internal entropy of a single image realization, if this can be defined, but with the full entropy of the phase space of all possible images. The discussion of this issue in our paper might have given the impression that we wanted to argue against the MEP, while we merely criticized the unjustified usage of MEM image restoration. We want to emphasize here, that we consider the MEP to be a correct and important concept.

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[1] D. Iatsenko, A. Stefanovska, and P. V. E. McClintock, Phys. Rev. E **85**, 033101 (2012).
[2] T. A. Enßlin and C. Weig, Phys. Rev. E **82**, 051112 (2010), 1004.2868.
[3] E. T. Jaynes, Physical Review **106**, 620 (1957).
[4] B. R. Frieden, Journal of the Optical Society of America (1917-1983) **62**, 511 (1972).